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MUTATIONS AND THE GEOGRAPHIC DISTRIBUTION OF NEARLY RELATED SPECIES IN PLANTS AND ANIMALS

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IN the *American Naturalist* for April, 1907 (vol. XLI, pp. 207-240) Robert Greenleaf Leavitt has discussed with great clearness and discrimination "The Geographic Distribution of Closely Related Species," with more special reference to plants than to animals, and also with reference to the bearing of the facts of distribution upon the mutation theory of de Vries. After presenting an impressive array of facts regarding the distribution of nearly related species, or "forms," in several widely different groups of plants, in part based on his own studies of the Orchidaceae, he gives his personal impression of the matter, stating in his concluding remarks that it seems to him "that the study of specific distribution in the vegetable kingdom is not likely to be unfavorable to Mutation, regarded as a method, but perhaps not the sole method, of evolution." He concludes: "The indications are that the adherents of Mutation will be able to bring forward enough cases of social distribution to render phytogeographic weapons useless in the attack upon this Theory."

Taking the facts of animal geography, as stated by a large number of zoologists, "we may say," says Leavitt, "that as a whole they militate against the operation of Mutation in a wide sense in the animal kingdom. This conclusion," he adds, "is not prompted by the attitude of certain of the zoologists mentioned . . . but is drawn from the geographic evidence."

In his final generalizations he offers this very judicial statement: "First, we note that zoologists and botanists are rather distinctly opposed to each other in their views of the actual state of specific distribution. The suggestion is offered that zoologists may best discover the condition and interpret its meaning among animals, and botanists among plants. In no case is it safe to reason deduc-

tively from one kingdom to the other. In the factors affecting their evolution plants and animals differ vastly."

Here is a concession from the side of the botanists that should do much toward harmonizing the conflicting views of botanists and zoologists respecting the influence of 'mutation' in the evolution of forms among animals and plants. In the first place the conditions of reproduction, structure, growth, etc., in the two kingdoms are so radically different that the methods of evolution may also well be different; indeed, it would be surprising to find them not so.

Mr. Leavitt's paper is primarily a contention that President Jordan's law respecting the distribution of nearly related species does not hold in the vegetable kingdom. It is assumed that Jordan's law was intended to apply equally to both animals and plants, which interpretation seems to be supported by the context of the paper. The law is as follows: "Given any species in any region, the nearest related species is not likely to be found in the same region nor in a remote region, but in a neighboring district separated from the first by a barrier of some sort." If we substitute in this expression the word 'kind' or 'form' in place of 'species,' and restrict its application to animals, it will probably meet with general approval on the part of zoologists.

In testing Jordan's law by an examination of the facts of distribution presented by the Orchidaceae, Leavitt says he "looked for pairs of kinds," and adds: "I say kinds instead of species intentionally. The main problem should not be confused by the difficulty of agreeing upon a definition of species. What the evolutionist has to account for is not the definitions of systematists, but the multiplicity of hereditary types; he has to explain the antithesis between the uniformity which heredity seems at first to promise, and the diversity which actually prevails among organic things." It is evident, however, that in the expression "pairs of kinds," the term kinds is given unequal breadth of meaning in different instances, and is not here the equivalent of "nearly related forms," or "subspecies," as these terms are employed by zoologists. As regards the higher vertebrates, the evidence is indisputable that two closely related forms do not occupy the same area. By this expression the subspecies of zoologists are meant,—in other

words, intergrading forms of a common stock. It is therefore perfectly evident that botanists and zoologists are often speaking of entirely different concepts when discussing the occurrence or non-occurrence of species in the same area. It is also evident that minor forms among plants bear no relation to the minor forms among animals, either in mode of origin or in manner of distribution. In *Crataegus*, *Rubus*, *Amelanchier*, *Viola*, *Aster*, and countless other generic groups of plants, there often occur many slightly differentiated forms growing side by side over large districts. Among animals, at least among vertebrates, no such conditions appear to obtain; the slightly differentiated forms occupy different areas, and where the borders of their breeding ranges approach they gradually merge the one into the other with the gradual change in the environment. In the case of the plants mentioned, these slight differentiations maintain themselves despite similarity of environment; in the case of the animals, they are obviously the product of environment. The origin of such plant forms may never be discovered, but to many minds their development by mutation may seem not improbable. So long as we do not find similar conditions among the higher animals, it is hard to see how mutation has been active in the origination of new forms, whether species in the usually accepted sense, or the minor variants usually recognized as incipient species or subspecies. With these facts and conditions in view Dr. Leavitt's above-quoted suggestion that "zoologists may best discover the condition and interpret its meaning among animals, and botanists among plants," is eminently worthy of serious consideration. It is "obviously unsafe," as he well says, to reason deductively from one kingdom to the other.

A recent re-reading of the various recent papers by botanists and zoologists on the subjects of "mutation" and the "distribution of closely related forms" in animals and plants has given me the impression that much of the opposition of views on these questions is due in part to too sweeping assertions by both botanists and zoologists, in part to a misunderstanding by one side of what the other side really means, and largely to deductive reasoning from wholly dissimilar conditions.